Beeman, Edward S. 2004

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Dr. Edward Beeman

Office of NIH History Oral History Program

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Buhm Soon Park: Today is May 4th, 2004. This is an interview with Dr. Edward Beeman. BSP is Dr. Buhm Soon Park of Office of NIH History. Today's subject is the research and careers of Dr. Charles Armstrong and Robert Huebner.

[break in audio]

BSP: Thank you very much Dr. Beeman for agreeing to have an interview with me. I'm just glad to hear about Drs. Charles Armstrong and Robert Huebner. Tell me – start talking about your careers, your educational and family background and when you came to NIH.

Edward Beeman: Okay. Yes, my full name is Edward Author Beeman. I was born May 1, 1923 in the Roxbury section of Boston, Massachusetts. I went through grade school primarily in the Boston area. I attended Boston public Latin school during my high school years. When I graduated from Latin school I entered a college at Washington Square College at New York University. I stayed there for one year and then I transferred to Harvard College and I graduated with a degree of Bachelors of Arts in 1943.

BSP: What was you major?

EB: My major was biochemical sciences. There was a lapse of about nine months, it was during the period of World War II, and I was then accepted at Boston University School of Medicine. I graduated from medical school in May 1947. I started my internship at the Massachusetts Memorial Hospital, which is now known as University Hospital. This was the university hospital of Boston University. I had a straight internship in internal medicine.

It was during that period that I met an assistant resident – or actually he was a fellow in medicine who knew of my interest in infectious diseases. This was Dr. William Hewitt [spelled phonetically]. He suggested that I might be interested in spending some time at NIH to get some laboratory experience in infectious diseases and he arranged for me to have an interview at NIH in January of 1948 with Dr. Charles Armstrong who was then the Chief of the Laboratory of Infectious Diseases.

I met Dr. Armstrong at that time and it was one of the outstanding experiences of my life because Dr. Armstrong was the world famous virologist and scientist. He introduced me to various members of the laboratory and during the course of the afternoon a young officer dressed in his official public health service uniform came into the room and Dr. Armstrong introduced me to the young officer who happened to be none other than Bob Huebner. I was somewhat familiar with Bob Huebner's early career since he had already published some findings that I found very fascinating.

BSP: Before getting more into the details of NIH years I am wondering you said that you had an interest in the research in infectious diseases and was it your intention to spend a couple of years at NIH and then coming back to academia or the general practice?

EB: Well, when I came to NIH I had planned to spend only several years and then I planned to go ahead and finish my clinical training in order to prepare for the specialty boards in internal medicine. I had originally thought about going into an academic career combining research with care of patients. I stayed longer than I had anticipated at NIH. I stayed a little over four years rather than two because I was frozen during the Korean War. And so after I finished my time at NIH I went to the Mayo Clinic in Rochester, Minnesota and had a fellowship in internal medicine – actually it was a residency program for a little over three years in preparation for practice.

I won't go into some of the things that happened during the course of my trying to become involved in academia –actually I had thought about coming back to NIH and working in the Clinical Center, but because of personal reasons I decided against it. Were there any other things that you'd be interested in as far as background?

BSP: I just want to know more about the reputation of NIH as a research institution among the medical students in the late 40's. You mentioned Dr. Charles Armstrong was a well known virologist and – but other than Charles Armstrong do you have – did you have any reservations about going to the federal agency or what about your advisors response to your decision?

EB: Well, actually I had no reservations about coming into a federal research institution. One of the reasons that I actually felt very fortunate in being able to come to NIH is when I was finishing my internship the physicians who had served as medical officers during World War II were returning to finish their training and most of them – or many of them were taking up the available positions for post graduate training in residencies and fellowships so the opportunity to come to NIH was really a unique one for me because this afforded me an opportunity to fulfill part of what I considered to be preparation for a future career.

BSP: I see, so during – when you were in medical school you were in the Army Specialized Training Program or…in the Navy program–

EB: Well, actually I was inducted into the Army in September 1943. I went to Camp Grant, Illinois for basic training and then [laugh] after spending a rather anxious month in December of 1943 the orders finally came through for me to be transferred to Boston University School of Medicine. So, I was very happy to be on the train from Camp Grant, Illinois to Boston.

And I was enrolled in the ASTP, or the Army Specialized Training Program. Now World War II ended with the surrender of Japan in August of 1945, but we were kept in ASTP until about March of 1946, as I recall, and then when we were discharged we were told that we were entitled to the GI Bill and also that we had no further responsibilities to the Armed Forces of the United States which did not turn out to be true as things developed.

BSP: So your coming to NIH was not motivated by fulfilling your obligations for –

EB: No it wasn't, but actually what happened – when I had my interview with Dr. Armstrong he told me that my position. or the position for which I should apply, was that off a post doctoral Public Health Service fellow and that's what my status was when I came here. However, seeing what was happening in the world I decided that I – and having an intuition that I was not going to be entirely without responsibility, I decided that since I was at NIH and I liked what I was doing I would apply for a commission with the Public Health Service and some time in 1949 I did apply and I spent the rest of my career at NIH as a commissioned officer.

[break in audio]

EB: Are we on now?

BSP: Yeah we are on.

EB: So, as I say I spent the remainder of my time at NIH which I left in September 1952, as a commissioned officer. I was in the Commissioned Corps reserve for several years after that, but then just resigned my reserve commission as well. So…

BSP: So, tell me a bit more about your later careers. So, you left NIH in 1950 –

EB: I left NIH in 1952 and I left the Mayo Clinic January 1, 1956 having fulfilled the requirements for training to qualify for the boards in internal medicine. Also, while I was at the Mayo Clinic, I did what all the other residents there did – I was a "graduate student" at the University of Minnesota and I had a little research project which involved looking at a bunch of slides of non-Hodgkin's lymphoma classifying them according to the current classification system then in use, wrote a thesis and I received a Master of Science Degree in medicine [laugh] from the University of Minnesota.

After I left the Mayo Clinic I moved to Detroit for a year. I was in a multi-specialty clinic, but it was not a very wise move and then I decided to return to the Washington area. During the interim period I had been in touch with people at NIH – actually he was – the person I'd been touch with was the chief of what we called the clinical laboratory of – actually at that time it was the National Microbiological Institute and we were deciding about whether I should return. Things happened, mostly to him, and I felt somewhat uneasy and I decided I would take my chances with the practice of internal medicine.

After I was in practice for about a year I was able pass my boards in internal medicine and I continued on with the practice of internal medicine, but also I started practicing the subspecialty of infectious diseases before the Board of Infectious Diseases was actually established. So, I was actually the first infectious disease specialist in Montgomery County, Maryland.

And then in 1972 when the examination was offered, the first examination for certification in infectious diseases, I took that and I became certified in infectious diseases. And then I just continued with my practice of internal medicine and consultative work in infectious diseases until my retirement January 1, 1993.

BSP: So, after you returned to the Washington area did you keep in touch with Drs. Armstrong and Huebner?

EB: Well, I say Dr. Armstrong and Dr. Huebner occasionally. As a matter of fact when we first came back – we came back in 1957 – I would go out to Dr. Huebner's farm in Fredrick County occasionally with my family which, by that time because I had married and I had three little daughters, and I used to see him quite frequently. We kept in touch over the years, but we gradually lost contact as I became more involved with my practice.

BSP: I'm wondering what made you interested in writing about Bob Huebner and –

EB: Well the thing is I've always been interested in medical history. As a matter of fact I'm sort of a history buff, general history, but medical history in particular and what happened is toward the end of my career, this would maybe be in the early 90's, I would talk to different people and they said, "What did you do before you started practicing?" I told them about my experience at NIH working with Dr. Huebner and Dr. Armstrong and when I mentioned the name of either one I usually I would draw blank stares.

Of course Dr. Huebner – Dr. Armstrong had passed away at that time. Dr. Huebner, of course, was ill with a disability caused by Alzheimer's disease, but nevertheless during the career of both men, they were the object of a great deal of publicity in the lay press and they also had extensive bibliographies and were well known in the medical community, but just a relatively a few years after they dropped out of public view, people had a tendency to forget about them. So I decided – I mean, not at that time, it wasn't until later, that I really shouldn't let the memory of the accomplishments of these men be completely forgotten.

This was just a germ of an idea even before I retired, but I really didn't do anything about it until I happened to meet Dr. Harden in the History of [unintelligible] Society of History of Medicine meeting, and when I mentioned I had worked with Dr. Huebner during the latter part of his career with Q fever, she said to me, "You know, I don't have any information at all about that particular – about the Laboratory of Infectious Diseases at that particular period," and she said, "Would you mind writing up your experiences?" I don't know whether you've had a chance to look at my own personal file, but I wrote of my experiences during that period, so anyway I turned it in and she seemed to like what she read and she said, "What are you going to do for an encore?" And at that time I really hadn't seriously thought about doing anything, but then as I kept talking to people and mentioned the names of Dr. Huebner especially, and I kept getting all this lack of recognition of the name, I said well, this is – you really can't do this, and then I saw Dr. Harden again. I said, "You know, I've been thinking about possibly writing a history of Dr. Huebner." So she said, "That sounds like a fine idea, why don't you go ahead and do it." So this was – I guess maybe about 1998. This was even before Dr. Huebner passed away. He passed away, I think it was August 26th, 1998, and I started collecting material. I knew his widow; she had worked at NIH. I had met her on many occasions. As a matter of fact she brought her son to me as a patient, and also she brought Dr. Huebner to me at one time, before he was hospitalized. So I started putting together some material together on Dr. Huebner. I started working at the National Library of Medicine, you know, getting some of his reprints together. Harriet Huebner, his widow, gave me his bibliography, and I used that as the basis to start gathering information about his life.

BSP: Did you have access to his personal letters, correspondence –

EB: Well, that's the – yes, well, actually this happened later. But when I talked to Harriet, I said, "Do you have any personal correspondence, or anything that you retained from the time that you were at NIH?" Well, unfortunately, she did not realize the historical importance of retaining this material, and they and eliminated a lot of this. However, later on she did refer me to one of the Huebner children, this was Suzie, or [unintelligible] Sue Huebner–Creamer. [spelled phonetically] And I went out to the farm, and she gave me three great big notebooks filled with newspaper clippings, and also a whole set of testimonials that were written by many of Huebner's colleagues, students and friends, that were really very helpful in putting together some of the information that I was able to glean about his interaction with colleagues and friends. But in any event, you know Huebner passed away and, of course, I think I saw Suzie Huebner after he passed away. This was after the memorial service that they had for him at the farm and, as I said, I started gathering the information, and then one day I bumped into Dr. Harden. I think it was over in building 10, I was going to a meeting over there. She said, "How are you coming along with Huebner's biography?" So I told her what I was doing and she says, "You've done so much, you must become official." So she made me a contractor. [laughter] And then I started using the facilities of the office, so I've been associated with the office ever since, and I continued the work, and everybody's been very helpful and very supportive in my ability to at least get a manuscript put together.

BSP: So and then you expanded your research to Charles Armstrong

EB: Yeah, well actually, I finished the manuscript on Huebner first, well actually, in the back of my mind I had always thought about doing one arm Armstrong, and I thought it was going to be easier to do the Armstrong biography first, because he had a much shorter bibliography, but then when Huebner passed away, I said, "Well, this is a little more immediate." So I decided to try to finish up with Dr. Huebner, and after I had completed that I started working on the Armstrong biography, and that's still in the early stages, but again I was very fortunate in having access to several great big notebooks that Dr. Armstrong's daughter, Mary Emma, had kept and it has a lot of anecdotal information, a lot of newspaper clippings and articles written by – well, actually there's a lot of information in there including some of the old official laboratories from the Public Health Service – actually, from the Hygienic Laboratory. So I have that material, and I'm using that to, you know, try to put together the Armstrong biography. Right now, I'm still on his early career. The thing I'm working on now is one of his research endeavors that he considered one of his major accomplishments, namely how to prevent tetanus from occurring in youngsters – people who were getting smallpox vaccination. He considered this a very important contribution, and I'm working on that at the moment.

BSP: Is there anybody who wrote about Armstrong and Wagner, [spelled phonetically] like biographical memoirs of the National Academy of Sciences? Both of them –

EB: Well, actually – Dr. Armstrong was one of the first members of – the first scientist at NIH who was elected to the National Academy of Sciences, and then Dr. Huebner, of course, much later on. As far as I know, no one has ever written a full biography of Huebner. As I said, there was a lot of – there were newspaper articles, there were articles in the New York Times Magazine about Huebner. Armstrong had someone who was very interested in him – this was the medical science writer Paul Duprief [spelled phonetically] who wrote a number of very interesting articles that appeared in some of the lay literature, the lay periodicals, describing just isolated segments of his work. I think there was one of his work on Psittacosis, and there was one on his attempt to protect the children down in Alabama by putting picric acid into the nose, but – and there was a lot of – he had a lot of newspaper publicity.

One of the things that was very important, that received a lot of press coverage, was the donation of Armstrong's blood serum that was flown by air to Mrs. Borah, the Senator from Idaho, because she was very sick from Psittacosis and Dr. Armstrong had recovered from Psittacosis. Well, in those days, what they used to do was if a person recovered from an illness, they would just take their blood and process it, [laughter] and give it to the recipient. Well, nobody thought about blood-borne pathogens. They were just interested in what was present in the way of antibodies. So anyway, Mrs. Borah recovered and she made a special visit to Washington to visit the Hygienic Laboratory, and she especially wanted to meet Dr. Armstrong, and there were a lot of newspaper articles showing her and Dr. Armstrong together, and she was thanking him for saving her life, but Dr. Armstrong was a very critical scientist, and he said, well he wasn't really sure that this is what saved Mrs. Borah's life.

EB: So, when you came to NIH, you specifically had in mind working with Dr. Armstrong, or somebody else?

BSP: Well, actually when I came to NIH, Dr. Armstrong introduced me to most of the members who were in the Laboratory of Infectious diseases at the time, and yeah, I met I think Dr. Havo, [spelled phonetically] Dr. Davis, Dr. Lawson, [spelled phonetically] Dr. Emmons, [spelled phonetically] and, as I say, the only person whose name I really knew was Huebner because I had read about his articles on the [unintelligible] and also I knew he had written something on Q fever, there was an – the second Q fever outbreak at NIH, and I thought originally that I'd, you know, like to maybe start studying rickettsiae first, so, and then anyway, as I said, I had on opportunity, it was a very fleeting moment, Huebner was in an out of the building – out of the office, in about 30 seconds we shook hands and Dr. Armstrong said, "Dr. Beeman is thinking about possibly coming down here." So Huebner said, "That's great." And he was off.

So anyway, at the end of the – after I had met all the men Dr. Armstrong said, "Who do you think you'd like to work with?" So I said, "I think I'd like to work with Dr. Huebner." And he said, "What do you think you want to do? Do you have any research project in mind?" I said, "No." I said, "I'll just go into the laboratory and I'll get started on something and I'll just see what develops." He said, "That's fine." As a matter of fact, this – I didn't realize that this is the approach that he took with Dr. Huebner. I think Huebner mentioned this, and then Dr. Armstrong said – shook hands with me and said, "You'll be hearing from me sometime in the middle of June," [laughter] and then he said, "And apply for the postdoctoral fellowship because that will be your status."

And so I was – you know, really – you know, quite impressed, elated, and then I got back to Boston, and you know I had taken a couple of days off to come down. One day to get down from Boston, one day for the interview, and then I took the train back to Boston the same night and I went to work the day I got back, and for that I had to work three extra weekends, a few extra days, and then this is – yeah, it got closer – this is back in January. Then around April and May, people started saying, "Gee, you know, you don't have anything in writing from Dr. Armstrong. Don't you think you better make sure you've got something set up, because come June 30th if you don't hear from him you're not going to have any position." So I applied for an assistant residency at the VA hospital, which was then in Framingham, the Boston VA hospital, and the chief of [unintelligible] was Maurice B. Strauss, who wrote the classical textbook on renal diseases, and around the middle of June I started biting my fingernails because I still hadn't heard from Dr. Armstrong. Then around the 15th of June I got a telegram, that said, "Come to NIH, report August 1st, 1948." The very next day, I had a telephone call from Dr. Strauss offering me the assistant residency [laughter] and I thanked him, and I told him, you know, I had to do something else. So that's how I got to NIH.

BSP: Were there many medical students or interns or residents wanting to come to NIH to have a research experience like you?

EB: Well, I don't think – well, I wasn't aware of too many in my school. Not too many planned to go into research. When I arrived down here I found that there were a few other fellows who were – I think were postdocs, one of whom actually stayed on, the other one, I don't know what ever happened to him. So there were a couple of us who were sort of in the same – had the same status, but back when I as graduating, the ambition of most of the graduates at that time was just to get their clinical training completed or, if they were going to go into a specialty, get their specialty training completed. But then, what happened to a lot of these fellows while they were in the middle of their training, the Korean War intervened and they were – they had to leave and go into the Army or Navy or whatever service they'd been affiliated with. As a matter of fact, my office associate had started a private practice. He had finished his training. He was trained in pulmonary diseases, and he had already – I think he had already served in the Navy for something like 20 months, and he was four months shy of completing his obligation to the Navy, so – he had just started up his practice, it had been going for about six months, he was called back into the Navy for another term of service, and so he got held up for another couple of years; he had to start all over again…

BSP: Could you portray NIH campus at that time and in what building did you work in?

EB: Well, I worked in Building 7 which was, at that time, the newest building on the campus. The campus was beautiful. It was like a small Ivy League school with lots of green space and Building 7 at that time was known as the Memorial Building and it had been dedicated to people associated with NIH who had lost their lives during the course of working in infectious diseases and the one who had passed away most recently was Dr. Henderson who died from scrub typhous. So, anyway, this was supposed to be the state of the art building. It was supposed to be a bio-safe facility, but it turned out that it was not. I mean it was designed to be that way. I don't know if you went through Building 7.

BSP: No.

EB: Anyway it was three floors and I guess there was a south wing and a north wing. And there were offices – a central administrative area, there were offices but there were a series of outer chambers that were sort of locked through which you were supposed to go before you went to the laboratory proper, the working space, you had to take a shower – go in and work, but no one ever did it.

BSP: Take a shower?

EB: Yeah. You were supposed to change clothes. As a matter of fact we all wore these coveralls. They were either a light brown or blue herringbone. As a matter of fact, I saved one of each during all the years I was in practice and then, before I became associated with the history office and museum, I donated these to the National Museum of Health and Medicine and I told Vikki about these coveralls and she said, "Oh, I'd love one of them." So, I said, "Well, you know, it might be a good idea to get the blue coverall, because that's the one we wore most of the time." So, anyway she negotiated with Alan Hawk down at the museum, who's head of collections, and after a little red tape he sent her one of the coveralls, but it was not the blue one. But anyway, they have a coverall from Building 7.

But anyway, I don't know if this is a true story or not but Dr. Armstrong was very skeptical about whether this building was truly germ proof and he did an experiment as follows: he took a bacterium called, Serratia marcescens which, back in those days, was thought to be just a harmless commensal and it since has been found to be a very potent pathogen especially in sick people who have a lot of instrumentation and immunocompromised people. And he took a culture of Serratia marcescens and put it into one of the exhaust ducts of the building and theoretically this stuff was supposed to be taken through a series of ducts and out through an incinerator grill that would destroy all the germs. But what Dr. Armstrong did was he left a series of open Petri dishes in the attic of Building 7. Now, this particular organism leaves a distinctly pink colony on a plate in agar and it was very – so a marker. So, anyway after a couple of days they went up and there were – I don't know how many Petri dishes he left, but every single one of them was just loaded with these germs.

So, he showed that the building was really not all that efficient and actually it never did prevent the third outbreak of Q fever but, on the other hand, the outbreak did not involve people working in the laboratory it was people who came in that didn't – these were young visitors, sometimes workers, from other areas came in and they wouldn't wash or change so they were exposed to the environment while we were working with Q fever and they came down. The only other thing is that the landlady of one of the workers came down with Q fever and it was felt that this person probably brought Q fever home from the laboratory because the landlady used to wash the linens, the bedclothes, the sheets and apparently she must have inhaled some of the organism. And then when she became ill her husband took over and he became ill [laughter]. So, anyway –

BSP: Were there animals and insects as well as bacteria and other…?

EB: Well, actually Q fever is one of the potential organisms that –

[audio break]

EB: – Q fever was actually from ticks out of the Rocky Mountain Laboratory Davis and Cox isolated Q fever.

BSP: So in Building 7 there were only researchers in infectious disease or other labs also?

EB: Right. Building 7 was – housed the laboratory of infectious diseases and all the men in there were associated with the laboratory. Now, also, infectious disease research was being carried on in Building 5. See everybody was in Building 5 before Building 7 was built and then when Building 7 was built some of the men moved over to Building 7. But Building 5 also had the Laboratory of Tropical Diseases and also had the Division of Biologics. So, if my memory serves me correctly, those were the major ones. Actually the Rocky Mountain Laboratory was apart of the Laboratory of Infectious Diseases at that time until it became an independent laboratory. I think it was when the Microbiological Institute was formed.

BSP: Was there any project going on for biological warfare or…?

EB: No there wasn't any as far as I know – I'm trying to remember. I think Fort Detrick may have been set up as a focus for the research on biological warfare.

BSP: But as far as you know Building 7 was not –

EB: No there was no biologics – no effort being expended on biological warfare agents as such.

BSP: So, when you finished your research you want to publish it in the journal and do you have to send it to someone to read it or screen it?

EB: Oh yes. Usually what happened – well, working in Huebner's laboratory he reviewed all the manuscripts – actually when we wrote manuscripts we all reviewed them, because I wrote a number of manuscripts in collaboration with Huebner. And then, of course, I wrote a number as senior author, but after we were satisfied that initial review by the authors was finished it was turned in to the chief of the laboratory. Actually, Dr. Armstrong retired as the chief. I came in August but he was – I think he actually retired in November of 1948, but he continued working in the lab, but Dr. Karl Habel became the chief. So, all the manuscripts went to Habel and then he would distribute them to various members of the laboratory for initial review and then – from then, once it had gone through the general review in the laboratory, then we would send it off to the journal where, of course, it was reviewed by their editors.

BSP: So there is no classified research or –

EB: No, nothing was classified. I mean everything was in open journals, at least at that time. I don't know about…

BSP: What are the main journals you wanted to publish in –

EB: Well, actually the main journals that we were writing in at that time was; *The American Journal of Hygiene*, *The American Journal of Public Health*, *P roceedings for the Society of Experimental Biology and Medicine*. We wrote a couple of review articles for *The New England Journal of Medicine* and *Journ al of the American Medical Association*, *The Journal of Immunology*.

BSP: What about *Bulletin of the Public Health Service*?

EB: Oh, as a matter of fact, my very first article [laugh] I submitted to the *Public Health Reports*.

BSP: And *Public Health Reports* has a good reputation as a scientific journal or just that you know a channel for –

EB: I think that it sort of functions the way *MMWR* functions nowadays.

BSP: What is *MMWR*?

EB: It was sort of an informational journal, but a lot of good research appeared in that because this was the primary organ by which the men associated with the *Public Health Service* would publish their findings. So many of the Dr. Armstrong's articles appeared in the *Public Health Reports* and also Dr. Huebner's articles, many of them, the early ones appeared in the *Public Health Reports*. And I don't know when the *Public Health Reports* stopped publishing, but I know it was after I left.

BSP: And, of course, you had frequent interactions with the people in Building 7, but what about the researchers in other buildings like NIAND had, at that time, Arthur Kornberg, Leon Heppel and also NCI had Dr. Green…

EB: Jessie Greenberg [spelled phonetically].

BSP: Greenstein [spelled phonetically].

EB: Greenstein.

BSP: And other people and there were a lot of biochemist and also –

EB: I don't think there was that much interaction. I used to go to meetings of what was called the Junior Officers Association and I would meet some of the young researchers at that time, but I don't think there was that much interaction between the people – you know the Laboratory of Infectious Diseases and other people. They all knew each other because it was a small campus but, again, they were working in different fields and they were pretty much confined to their own particular research projects.

A lot of what was – a lot of the research that was done at that time was sort of strictly biological and I don't think anybody was really doing anything in the chemical or physical sciences related to infectious diseases, at least while I was here. Later on there were some people out at the Rocky Mountain Laboratory who started becoming interested in other – applying other disciplines to the study of infectious disease problems, but this was well before – well, it wasn't that far before, you know, the DNA technology and, you know, the working out of the structure of DNA by Crick and Watson. That was 1953, so when I was here there was very little interaction or application of related scientific disciplines.

BSP: I don't know whether you sensed at the time, but after World War II, about a decade after World War II, there was a change in emphasis from infectious diseases to chronic diseases like cancer, heart and other –

EB: Oh yeah.

BSP: – things that while you were working in Building 7 did you fee,l well, NIH was really changing its emphasis? Did you feel that somehow –

EB: Well, I didn't. I didn't feel it personally although I knew that this was going on. I'll tell you what happened. See, I grew up in the era of the introduction of antibiotics and everybody said, "Oh antibiotics – infectious disease is a dead science."

BSP: [laugh] Did they [unintelligible] opening?

EB: Oh yeah, yeah. It was the end of history, you know?

[laughter]

EB: And I remembered – let's see when was it? I know I came up for some examination I think it may have been for one of the license examinations and I met one of my college classmates, he says, "What are you doing?" So, I said, "Well, I'm working at NIH in the Infectious Disease Lab." He said, "You know, that's an obsolete science now." He said, "With antibiotics we're curing infectious diseases." So, I said, "We'll see."

EB: [laughter] So because that's what [unintelligible] said then.

BSP: What about Charles Armstrong? Charles Armstrong you said had resigned from his position as a lab chief in 1948. Is there anything to do with it?

EB: When I came down here he was still a pretty vigorous man and there were some political machinations going on and –

BSP: What kind of political –

EB: Well, I – this is something that I've heard rumors about and information I got through the grapevine. I don't think there's any documentation for any of this so, I don't want to go into this in any detail, but I think that Dr. Armstrong was a victim of political maneuvering. I know there were several people who felt that he may have been sort of outmoded or past his time. He was still a pretty sharp –

BSP: This is a general mood about –

EB: I'll tell you. If you turn this off for a second, I'll tell you.

[break in audio]

BSP: Okay, can you tell me more about Charles Armstrong and his accomplishments and his leadership in the laboratory, any other things you could observe at the time?

EB: Well, my actual observation of Charles Armstrong was somewhat limited because he was not all that active, but I had an immediate favorable impression when I met him for my interview, and then when I came to NIH, when he saw me, he said, "When you get ready to start working you can order anything that you want." As a matter of fact, Dr. Armstrong had the reputation of wanting a very well organized and ordered laboratory that was economical. As a matter of fact, he was somewhat – I don't know what to say – envied or admired, because at the end of the fiscal year, he always had money left over and the administrators didn't know what to do with the extra cash. [laughter] But he ran a very efficient laboratory, but there was never any problem getting the necessary equipment to do the work, you know, as long as it's an approved project.

BSP: Is he an MD or a Ph.D.?

EB: No, he was an MD. As a matter of fact, Dr. Armstrong was a – graduated from Johns Hopkins Medical School back in 1915, and one of his very famous classmates was Dr. Thomas Rivers, a well known, actually he was the dean of virologists in the 20th Century, and Dr. Armstrong actually wanted to go into private practice, but then he started thinking about all of the financial obligations that he would incur, including – he said he would need a wife. [laughter] So he decided he'd better do something else.

It was during his internship that he saw a notice for examination for the Public Health Service, and he decided that maybe he ought to do this. So this was back in 1916, and so he applied for Public Health Service and – in the early days the examination was really very difficult, and admission into Commission Corps was very selective, I mean they just took the best, and he passed, and then he went through a series of assignments, like any junior officer, he was stationed at Ellis Island for a while examining immigrants, and then when World War I broke out he was assigned to the Coast Guard, and he was – he actually saw wartime service, sea duty during World War I. He was also involved in treating seamen, Coast Guard seamen, during the influenza pandemic of 1918, 1919, and spent his early years studying influenza, but one of the things that kind of thrilled me a little bit is when I started doing his biography, relates back to one of my medical school experiences.

During my bacteriology class, our professor, a very wonderful lady, was giving us a lecture on botulism, and she wanted to describe the extreme lethal potential of botulinum toxin, and she mentioned an epidemic of botulism caused by spoiled ripe olives, and she was trying hard to – just a small amount of one of these spoiled ingested olives would result either in severe illness or even death, so when I started doing Armstrong's biography, his very first publication was a description of this epidemic of botulism in Ohio. This is when he was acting as an epidemiological aid to the state health officer of Ohio, and then he had several – then he investigated an influenza epidemic in a closed community on an island in the middle of Lake Erie, and it was after these two episodes that he was invited to join the Hygienic Laboratory, then he started doing his work there. And I mean, he just went ahead and did major, you know, research.

Of course, the thing that he considered his first major contribution was the illumination of tetanus as a complication of smallpox vaccination, and then he wrote a little bit about a post – vaccinia encephalitis, and then of course he was involved with the psittacosis epidemic, I guess around 1930, and it was during – it was while he was investigating psittacosis that he became ill, but also he discovered that psittacosis was a filterable agent. They never visualized it, they never grew it, but apparently they did not – at least in the laboratory at that time they didn't have the tools for isolating that particular organism.

The next major thing that he did was isolating the virus of St. Louis Encephalitis, and while he was investigating that, he discovered an unrelated virus from a patient who died during that epidemic, and it was from this patient that he isolated the virus of lymphocytic choriomeningitis, and this was really an amazing accomplishment considering that he noted the difference on the basis of the pathologic change in the brain. Of course, the people who worked in the Hygienic Laboratory were really very highly skilled specialists in almost everything, and Armstrong also happened to be a very excellent pathologist, as well as an epidemiologist. Then he was the first one – he found the reservoir, which was in mice, and also he described the – this is the lymphocytic choriomeningitis – as a flu-like infection in humans, and also he – I said he found the reservoir in mice.

This brings him up to about the period of the middle to late 30's when he started becoming interested in poliomyelitis. Working in the laboratory, he found that he could block the spread of certain neurotropic viruses in mice by instilling astringent material into the nasal passages because apparently, by blocking the root of the olfactory nerves, he could prevent the virus from penetrating the central nervous system, and he thought that this was also the way that polio might be transmitted and, apparently, this was really not the main mechanism, but anyway he speculated that if he could block the nasal passages with an astringent material, he might be able to influence an epidemic of polio, went down to Alabama, started putting picric acid into [laughs] the noses of people, but the epidemic got out of hand, the way the insurrection in Iraq is getting out of hand. I mean, people just started spraying each other, and there was no way that he could run any kind of controlled experiment.

BSP: Do you know what he did during World War II?

EB: Yeah, well, actually right before World War II, I think it was 1939, he was able to adapt type 2 polio virus to rodents. This was a major advance because before that people working with polio had to use monkeys; it was the only known experimental animal, but then he adapted polio first to the cotton rat, the eastern cotton rat, and then to mice and, for ten years, this was the only experimental animal other than monkeys that could be used to check – do research on polio, and it was – as a matter of fact, it was one of the mouse adapted polio – one of the Lansing type strains, type 2, that Enders used, you know, to work – cultivate the polio in non-neural tissue, Enders, Weller and Robbins.

But during World War II, I think Armstrong was primarily – actually, back in 1940, he was involved in the first epidemic – or the first laboratory outbreak of Q fever. He became ill himself. He described some of the pathology of Q fever and then, I think during World War II, he mostly just administered the laboratory, but then he went out to Rocky Mountain Laboratory to help with the production of yellow fever vaccine, and it was while he was out there – somehow or other, he was exposed to tularemia and he almost died from tularemic pneumonia and, as a matter of fact, his daughter and wife were on constant alert to be ready to go up to Montana at any time. But anyway, fortunately he recovered from that, and so – then – let's see, that was back around 1942, and then the war was still going on. In 1944, he recruited Huebner into the laboratory, and he helped Huebner with the rickettsial pox study, and – I don't know whether you – did you read the chapter on –

BSP: Rickettsial, yes, yeah.

EB: And the way that Armstrong immunized mice against lymphocytic choriomeningitis, because, see, the colony in Kew Gardens, the mouse colony in Kew Gardens had endemic lymphocytic choriomeningitis as well as hosting the agent of rickettsial pox. When Huebner tried to – they got some mice from that area, and tried to inject other mice – laboratory mice, they came down with an illness, but it wasn't rickettsial pox, it was lymphocytic choriomeningitis, so Armstrong immunized a group of mice against lymphocytic choriomeningitis and then, after these mice were immune, then their tissues were injected into laboratory mice. That's when they were able to isolate the rickettsial pox. So that was what he did. After that, he really didn't do an awful lot in the laboratory. He collaborated with some of the practitioners in the area. I think he isolated toxoplasmosis from a lymph node. He became somewhat interested in cat scratch fever. Then, actually in retirement, he started doing some atmospheric studies to see if he could correlate changes in temperature and humidity with the prevalence of polio, and I don't think this work was every really of any signifcance. Most of his really significant research studies occurred, I guess before World War II.

BSP: Could you comment on the size of his laboratory, and how many principal investigators are working with him, and how many technicians, and –

EB: Well, actually, he –

BSP: Or by himself –

EB: Well, in the Hygienic Laboratory, he worked with several other people. He collaborated quite a bit with Dr. James P. Leek, [spelled phonetically] who's head of the epidemiological unit. He worked with him during the St. Louis Encephalitis period. He worked with occasional other members of the Hygienic Laboratory. He usually had, I think, one laboratory technician of his own. I think when he became chief of the laboratory, it think it was 1942, I don't remember the exact number, but whoever was in the Laboratory of Infectious Disease he was administering, and that included the rickettsial unit, which consisted of Topping, Shepherd, and then Henderson, before Henderson died, and then Huebner, and then there was Dr. Carl Larson, Dorland Davis, Chester Emmons, Sam Salvan [spelled phonetically] and, of course, Dr. Huebner, Dr. Bertsel Carl [spelled phonetically] who was head of the Brucellosis Unit, so he had maybe about a dozen investigators who were part of laboratory, and he administered [inaudible].

And he would go in and sort of putter around the laboratory, but the one thing he did is when we started the work on the coxsackie viruses, Bob Huebner wanted to make sure that we weren't dealing with any Polio, so Dr. Armstrong I think took some of the material from our epidemic – things that we knew, Harvard, coxsackie viruses, injected them into monkeys, and showed that there was no polio, so he was the one that helped exclude polio while we were studying – and then, one of the things that really was a major observation was noting the difference between coxsackie A type 1, and the other herpangia viruses. I don't know whether you have a copy of the paper that I wrote on the laboratory aspect. We have pictures of the differences in the appearance of the mice, the ones –

BSP: Yeah –

EB: So he pointed that out, and also, it was shown histologically that there were definite differences in the appearance, but the thing that was really the payoff for this was that, at least from my perspective, was when I was asked by Dr. Huebner to start working on my own little project and take over the pleurodynia project was recognition that the first isolates we had were of this group A type 1 and we knew that this was probably a contaminant because during the course of the herpangia studies we knew that people could be infected with more than one enterovirus. So, sure enough, we neutralized the mice that had the coxsackie A type 1 and passed their tissues into other mice and we isolated the coxsackie group B type 3, which was a cause of the pleurodynia epidemic. But it was that observation that Dr. Armstrong contributed that was a tremendous help in, you know, helping to elucidate that particular problem.

BSP: Right. In your book on Huebner you mentioned a little bit about the relationship between Armstrong and Huebner. Huebner – why Huebner came to NIH and he had an interview with Armstrong and there are several other versions. Could you start talking about Huebner and when he came to NIH?

EB: Well, yeah Huebner is very remarkable from the standpoint that he really had no scientific training. He came – he had medical school training, but the thing is that Huebner started off with a great mind. He was in elected to the honorary society in medical school and he had his sea duty with the Coast Guard up in Alaska, but nothing outstanding. He was just a good officer, a good medical officer. And then, when he was transferred from Alaska, he was put into the ear, nose and throat clinic downtown at the Public Health Service headquarters and I remember talking with Huebner. I said, "How come you came here?" He said, "I was interested. I wanted to do something different. I thought I'd like to do a little research," and he said he went to a commissioned officer's gathering and he met Dr. Armstrong and he just talked to Dr. Armstrong and wanted to know if there were any opportunities out at NIH and Dr. Armstrong said, "Well, come on out and I'll show you around." [laughter]

EB: We were so matter of fact. I mean this may not be the exact quotation, but basically he became acquainted with Armstrong. He came out here. Armstrong interviewed him and Armstrong took him on. As a matter of fact, I was talking to Mary Emma [spelled phonetically] Armstrong and what Huebner wrote in his letter is at some variance with what she remembers her father telling her about Huebner's arrival at NIH. Apparently Armstrong came home and he would share some of the information with Mary Emma] the daughter and his wife. He says, "A new young fellow came out," and he says, "He's a bit of a smart-alec. We'll have to teach him a thing or two."

EB: [laughter] I may put that in the Armstrong biography because it is a little bit of – so you see there is some variation in oral history about what's spoken and what people hear, but in any event I think what Huebner tells – relates about the way he was started here was probably pretty true. The fact that he was just put into a room like this, maybe a little bit larger, and told, "Well, here's your lab and office; get started." And he went scrounging around, found some furniture and picked up, I guess, some test tubes and beakers and all that stuff and then, apparently, he must have been taken on by the Rickettsial unit actually to replace Dr. Henderson who had died of Q fever.

Something that wasn't in the Huebner biography is that there were two people who came down with scrub typhous, and Henderson and also his laboratory technician Leroy Snellbaker [spelled phonetically]. And apparently Henderson was this great, big, healthy, rather large person and Snellbaker [spelled phonetically] was sort of a not a very tall person, kind of skinny guy, kind of scrawny. Snellbaker [spelled phonetically] survived and Henderson, of course, passed away, but during the last couple of years that I was at NIH Snellbaker [spelled phonetically] was my lab tech. He was great, really was.

BSP: Let me stop our tape one and I will change the tape.

EB: Okay.

BSP: Thanks.

[end of tape 1 side B]

BSP: This is tape 2 of the oral history interview with Dr. Edward Beeman.

[audio break]

BSP: So, you mentioned that Dr. Huebner had relatively little research experience before coming to NIH.

EB: Absolutely none, yeah.

BSP: And by the time you came to NIH in 1947 / '48 did you see Huebner establish himself as a researcher?

EB: Well, let's see, he came in 1944 and I came in 1948. He already had one major research accomplishment. That was the Rickettsial study. He had participated as a member of the Rickettsial unit in another study it was a comparison of the immunologic properties of six Q fever strains. It was during the course of this study that the second outbreak of Q fever occurred in laboratory. This happened over in Building 5 and Rob Huebner studied the epidemiology of that outbreak and he correlated the incidence of the outbreak – of the incidence of cases in the outbreak with the activity during the preparation of Q fever antigens, which produced tremendous contamination in the environment. He also did a clinical study of the cases who were hospitalized over in, I guess, the Marine Hospital in Baltimore, so he studied those.

So, he had as I say that – as a matter of fact it was the Rickettsial study, and also the Q fever outbreak, that I became acquainted with before I came down to NIH; as a matter of fact I think I remember reading about the clinical aspects of Q fever. When I came down here in August 1948 Rob Huebner was in California working on the Q fever in the dairy industry in Southern California, and this was also his second major accomplishment. As a matter of fact, this was probably some of the best work done in Q fever since people started studying it. As a matter of fact, I arrived in August and he didn't show up until either October or November so for three months I never saw him after I arrived here. And when he came down what he did was he said that they had three new strains that they wanted to check against the six original strains that they had studied – the Q fever strains that they had studied back in 1944. And so this was the project that I got started on and that's when I became involved in learning how to deal with Rickettsias laboratory techniques, growing them in eggs and immunizing guinea pigs and making antigens and all that and –

BSP: So, by that time Huebner was quite experienced –

EB: He was –

BSP: – on top of everything?

EB: He was a very experienced investigator by that time. As a matter of fact, it was very interesting when they started working on Q fever. He went out to the Los Angeles area, originally with Dr. Charles Sheppard who was a very brilliant guy, but mostly a lab man; he really didn't like to do epidemiology, and they uncovered the fact that Q fever cases were occurring in relationship to the dairy industry in Southern California. So, anyway they felt that this had to really be investigated because they felt it was a disease of major public health significance, but Sheppard didn't want to be involved any longer so Rob Huebner took over the entire project and it was at this time – you know, this is when Dr. Armstrong really gave him tremendous support in terms of personnel, funding for the project and also, apparently, he got him out of a lot of administrative difficulty because Bob had the habit of – if he needed something while he was out in California he'd just go ahead and buy it and then send the bills back without consulting with anybody. He got into all kinds of [laugh] administrative hassles and apparently Dr. Armstrong, Dr. Habel, got him out of all these administrative quandaries. And also, as I say, Dr. Armstrong gave him tremendous moral support in this activity and it was also at this time that Bob Huebner began collaborating very actively with one of his other major collaboratives, namely Dr. Joseph Bell who was head of the Epidemiology Unit, Laboratory of Infectious .Ddiseases.

BSP: That's what I want to discuss a little bit in detail. How Huebner's work / research got started. There is an outbreak – epidemic outbreak in some area in the United States and then he was asked to go there and examine things and come back to the lab and he'd do the lab work, and so it seems like there was a combination of field work and laboratory work and epidemiology work and etiology work and other chemical experiments. Could you explain the flow of the research work and how it got stated?

EB: Well yeah, this is actually a continuation of the tradition of the hygienic laboratory. The men combined field work with bench work and –

BSP: And sometimes clinical.

EB: And occasionally clinical work if they had the experience. Now Huebner had clinical experience so he did clinical work early on, but the Q fever epidemic started when one of the practitioners in Southern California – in Los Angeles county, I think it was Dr. Young, saw a lot of patients with atypical pneumonia and he suspected Q fever. He sent blood to NIH, and more than one sample, and these bloods came back positive for Q fever antibodies and then I guess the local health department became concerned because actually one of the health officers came down with Q fever. So, what happened – now this is the days before CDC. Any request from the states for federal help would come to NIH, and usually to the laboratory of infectious diseases or its previous entity the division before that the hygienic laboratory. And most of this help was to help with an infectious disease.

So, anyway, the word got to NIH and so they sent out a team, the initial team were Sheppard and Huebner. And, you know, they started looking around and seeing cases, getting specimens from them, which they sent back to NIH, and then they tried to see what the epidemiological factors were. They looked at the dairy industry in Southern California which was rather unique, I think, as I discussed in chapter on Q fever. And, so anyway, they came to the conclusion that this was a disease of major proportions and somehow it was related to the dairy industry and so Bob Huebner felt that this was something that really had to be looked into and, as I say, Sheppard decided he just wanted to work in the laboratory. So, Bob Huebner went out and he organized this program, enlisting the help of some of the community physicians, the public health people at the county and state level and, you know, they would try to do case studies and then, also, he realized he had to have epidemiological help, so this was when he called Dr. Joe Bell, and Joe Bell, in turn, got some of the state and local epidemiologists involved. So this turned out to be a rather large-scale study of an outbreak involving a lot of people, and Huebner sort of administered the whole thing, I mean he –

BSP: Could you comment more on the collaboration between epidemiologists and virologists, in other words Bell and Huebner, and what kind of information epidemiologists provide for –

EB: Well, the epidemiologists would go out into the population and –

BSP: Do the interviews?

EB: They would do interviews, and physicians would collect specimens of urine – they would collect blood specimens for antibody studies. In the meantime, Huebner was collecting material from the cattle – he was the first one that tested the milk and showed that –

BSP: Oh, I see.

EB: So, in other words, it was a collaborative – everybody worked together, and they pulled the information and correlated the results say between blood, isolation of organisms, history of illness and with various epidemiological factors – occupation, proximity to the sources of infection, like whether they lived near a dairy or far from a dairy, and what they would do is they would also try to have enough of a population to act as a control. They used a large group of normals as a control to the patients who were infected, and this was the same principle we used when we started doing the studies on the coxsackie virus; basically, it's the same thing. So the Q fever study was done on a tremendous scale compared with the smallest scale that we used, when we went into the – at least the herpangia studies. Clearadinia [spelled phonetically] studies were also done on a large community basis and again, you know, we used more people, but the principles were the same. Half the patients with an illness – try to get material to isolate the organism, or the causative agent and then, if it's something that's deemed to be of public health importance, then you get the epidemiologist in to try to pin down what are the factors that bring about the establishment of an epidemic of this particular organism.

BSP: What about the clinical, such as it seems like the next project, or the following project, was done in the junior college – Junior Village, and it seems like –

EB: Well that came, actually that started in 1955, so this was a few years afterwards.

BSP: But this case –

EB: But there are some other things that happened in between that and Junior Village. So anyway, Huebner was able to definitely establish that Q fever in dairy cattle was spread by means of the milk, and then later the veterinarian Laurie Luwoto [spelled phonetically] showed that the placentas of birth membranes also were heavily laden, so this was another factor; this accounted for infection among people that worked in tanneries, worked with hides, so all of these things together built up a picture of Q fever related to the cattle and their products. Milk, and hides and things like that, so this is the way that they established the connection. Interestingly enough, when CDC became established and started having the epidemiologic intelligence service, NIH really just stopped doing these sorts of things. The only time they really did these kind of population studies when they started developing vaccines, but in between this and Junior Village, there was – well, actually what happened after Huebner finished with his studies in California, as I said he was sort of kicked out of the state, because the men who ran the certified dairies were very upset, [laughter] so he said, "You go down." [?]

Anyway, when he came back to the lab, this little project I had – we never wrote it up because it showed basically the same results that the original study showed, and we figured that it just wasn't worth publishing. Beside which, we never really tried to get into the reason why did this happen, and it was actually somebody out in Rocky Mountain Laboratory, Dr. Richard Ormesby, [spelled phonetically] who found that the difference in the immunologic behavior of the different strains was due to whether or not it was a phase 1 or a phase 2 organism, but that's another story. But in any event, they say that this was sort of thing that Huebner initially was not interested in. He was interested in the big picture, and [unintelligible] on the other hand started employing bio-chemical methods to tackle this particular problem.

But the next thing that happened was Huebner came back from California, having been kicked out, and apparently he started thinking he wanted to find out what were all of these quote unquote virus infections that they were producing respiratory symptoms, and he wanted to work on those, but he really didn't have the background to do this. But instead, Coxsackie viruses were just becoming recognized, so Joe Bell took us up to a meeting in Pittsburgh, and we heard about Coxsackie virus and we came back, and this was around late summer, and this was typical of Huebner's thinking. He said, "You know, this – these viruses tend to follow the temporal occurrence of polio." he said, "We're entering polio season now." He says, "What we should do is we could maybe handle some small epidemics that occur locally that we can study." So, he said, "Let me go around and talk to a few people in the laboratory, and most of them live in the area, and see if they know of any epidemics."

So he started – we used to have – we had a room about – a little larger than this one where we used to eat lunch together, so we would come into the lunchroom and ask the man, "Have you heard of any epidemics lately?" [laughter] – of cases of fever that are spreading. So one day, one of the men said, "Gee, you know, there are a couple of kids ill over in Parkwood," this is a little area right across Wisconsin Avenue, going up Cedar Lane. So Bob said, "Well, why don't we go and see what's happening?" So the first thing we did is we got hold of a nurse and some material to collect stool specimens. We started collecting stools from these people and Bob said to me, "Ed, you're going to start isolating viruses from stools." So I said, "Okay, fine." I had never worked – I had just been working with eggs and guinea pigs, and hadn't worked with suckling mice, so I had to start from scratch to learn how to work with these animals, and learn how to harvest their tissues in order to get the proper reagents.

So, lo and behold, I think there was an epidemic of something like eight cases in the first year. Every time we'd put specimen from the stool into the suckling mice, they came down with paralysis, so we put them in the freezer, and we kept collecting stools, and then Huebner and Bell organized the epidemiological study and the community survey, and it was at this time that my classmate from medical school came down and he joined me; he had spent a year up in Boston working with Louie Weinstein. So Roger went to work with Joe Bell, the epidemiologist, and Roger really did the epidemiological work on the epidemic, so in the meantime they had nurses and I think Roger went out – while Bob Huebner went out examining patients and, in the meantime, they were collected stool specimen, they started bleeding people, and all this material was coming in was being logged into the laboratory, and stored in the deep freeze until we could get to it, and then we started, you know, putting these specimens into suckling mice and before you knew it we had these isolations and then, after a while, we recognized that there was more of a – there were different kinds of viruses floating around – the first outbreak we isolated with just a single serologic type. But then the subsequent isolations we found were not associated, and apparently these were just in the community.

So anyway, we – I – as you might say, I mined gold out of all this fecal material and we, you know, wrote up our experience for the first year and then I was sent out to San Francisco to deliver our first paper, and we didn't – hadn't recognized the illness as such because we didn't see the cases really early when they had the typical symptoms that we recognized with the second outbreak. So I gave the paper out in California. Then, also, I monitored the Q fever exhibit out there. This was in June of 1950, and all – I was in San Francisco, the Korean War broke out, but fortunately [laughter] I was in the Public Health Service. So I was actually taking six weeks – it was a combination of leave meaning time, and that – it was a six-week period, and I was about halfway through the six-week period, I guess I had another three weeks to go, so I called up the office and I said, you know, "We're at war." I says, "I'm here in San Francisco, and I have three weeks left on my leave, shall I come home immediately?" They said, "No, take your time."

As a matter of fact, on the way back, I had arranged an interview with the Mayo Clinic. [laughter] So, after we left San Francisco, took a slight detour to Yosemite, [laughter] and then we went up the coast of Aragon, went through the Redwood highway, and then we went up the Columbia River valley, and then we stopped off at Glacier National Park and went down, we stopped at Yosemite, and then we started – headed for home, I stopped off at Rochester, Minnesota, had my interview, and then we came home. And I no sooner came home when we heard about another outbreak of illness at Parkwood. This time, however, Dr. Carl, [spelled phonetically] who was head of the Brucellosis Unit, his daughter was the first patient, and Dr. Carl, [spelled phonetically] being a compulsive physician, looked down his daughter's throat because she was complaining of a little sore throat, and he noticed the spots that were very typical of the condition that we call herpangia. He called up Bob Huebner, and he said there were some other cases also that were the same thing. He said, "You ought to take a look at them." So Bob went out, and he saw these other cases of herpangia, and then there were some other scattered cases, so that's how we happened to –

BSP: By the time, you know, the CDC was created –

EB: Well, CDC was created, but they weren't really into looking into these epidemics. NIH – the Laboratory of Infectious Disease was still doing, I would say, most of the federal assistance at the local communities, if they wanted help, you know with investigation. CDC started, I guess it was maybe the middle of the 1950's decade. Actually, I think one of the last community outbreaks that we investigated, or that the laboratory investigated was the outbreak of pharyngoconjunctival fever caused by adenovirus. Bob Huebner and – this was back, I think this was around 1954.

BSP: So, about – before that time, when there was a – outbreak, NIH scientists were asked to investigate, and then if it happens to be Huebner, Huebner goes, and if it happens to be others…

EB: Yeah, they would be called to investigate, say, tropical diseases, the men from that lab would go. But often the states had their own laboratories, their own epidemiological expertise, so they could do it without calling on the federal government for help.

BSP: I think it's very interesting that the research, original research, was initiated by some epidemic outbreak, or call from [unintelligible] service, and then serious lab work and field work going on, and then after it's finished, scientists came back to their labs, and was there any ongoing project while the people like Huebner were away?

EB: Yeah. While Huebner was in California; they were sending all these specimens back. Now, while we were working on Q fever, Bob had a project going with his chief bacteriologist, Betty Ransom, [spelled phonetically] and he was interested in the resistance, so coxsackie [unintelligible] to various chemical and physical agents. They found it was very resistant. As a matter of fact, one of the things he found was the Q fever organism resisted temperatures that were used in the normal process of pasteurization – commercial pasteurization.

The other thing that he started to do – well it wasn't with Q fever, it was when we got into the coxsackie viruses he started becoming interested in the physical properties of the coxsackie virus, or at least the group A viruses, and we had a guest worker from Germany, Angelo Breeves [spelled phonetically] who had a PhD in, I guess it was maybe biochemistry or physics, so anyway I supplied the material and what they did was they did electron microscopy studies on the organism. They grew just the one strain of group A coxsackie virus grown in mice and then also we had adapted a strain to eggs so they compared the two so they studied the electron microscopic picture and examined the size that way and then they did sedimentation constants and measured the size that way.

They didn't do any special chemical studies, just these physical properties that they were really interested in, but this was really the first time that Huebner started doing anything of that sort. Before he was interested mostly in the biological aspects of the organisms and then, of course, later on as time went by, as he had the people who had the interest and also the training, that he was able to go and get into the biotechnology age and it was with the appearance of Wally Rowe that he really started – became interested – he was able to really study the organisms in greater detail.

And as a matter of fact right after we finished with the group A coxsackie viruses I became interested in the question of the age susceptibility of mice and I started doing some preliminary experiments and in retrospect now I realize that this probably – I was taking the wrong approach. I don't think it's ever really been shown definitively why there's a difference in age susceptibility. There have been some papers written on whether the development of interferon in tissues helps protect mice of a certain age or whether the loss of phago-receptors at a certain age also prevents the virus. So, those were some of the approaches that have been taken, but I don't think I've seen anything really definitive about that.

BSP: So in early 1950s he was still working, field work and the laboratory work and then –

EB: Yeah.

BSP: – and here is a case of I really want to go to the '60s and his virus cancer program, but before that there was another community-based research Junior Village –

EB: Well, actually what happened is when I left Huebner decided that the only way he could study respiratory viruses was to have tissue culture capability.

BSP: Okay.

EB: And fortunately that's when Wally Rowe came in, but it was again Huebner's intuition that prompted him to use explants of what appeared to be normal adenoids and tonsilar tissue to put into tissue culture, and it was from these normal explants that they isolated the adenoviruses and then, of course, once they had somebody who was acutely ill there's no problem isolating them, but Huebner – Well, Wally Rowe really set up the tissue culture system in the laboratory. Dr. Alex Shelokov [spelled phonetically] who was working with Habel actually set up the first tissue culture the roll-a-tube [?] culture system at LID. He was sent up to Harvard to work in Enders's laboratory to learn the setup and learn the technique. He came back then he instructed Wally Rowe about how to do it. And, as a matter of fact, I think I've got this all outlined in the section on the adenoviruses.

And then Bob Parrott recognized the clinical entity of pharyngo conjunctival fever and that was one of the first clinical infections that was studied in the Clinical Center back in 1954 and also the outbreak of pharyngo conjunctival fever was the last community outbreak that Huebner and Bell worked on and it was after that they conceived of doing a longitudinal and cross-sectional study of the prevalence of infections in a closed population group and that was what the junior study did and, of course, from that they isolated all kinds of viruses and –

BSP: So it's going to the community and it is examining what kind of viruses are there?

EB: Well, actually it wasn't a community. It was a closed population. Before that Huebner was in the general community out in the population, but here he was taking a group of nursery kids who were confined to a hospital ward or a nursery ward, and they were checked on a periodic basis of – they'd collect the specimens from them on a regular schedule and bled them at scheduled intervals and they isolated all these different viruses and as a matter of fact they isolated so many of them that sometimes they couldn't correlate which virus [laugh] with what disease.

BSP: Virologist's dilemma?

EB: Yeah, that's what Huebner philosophized about how to tell when a virus is causing a clinical entity. As a matter of fact Vikki Harden paraphrased this in one of her publications and it was after this time – about this time, towards the end Huebner's major involvement with the Junior Village Study – the first three years were the most active and around 1958 he became interested in working on the cancer viruses.

BSP: So, in the Junior Village he mainly collected viruses –

EB: Well he –

BSP: – rather than doing any clinical trials or other…?

EB: Well, they didn't do any clinical – no actually they did some clinical trials. Joe Bell was interested in the effects of penicillin on some strep infections, also he was interested in checking different vaccines. They had a full time pediatrician who examined the kids on a daily basis. So, Huebner actually didn't do detailed clinical examinations. As a matter of fact, they collected so much material they had deep freezes crowding out everything else. As a matter of fact that's when the major physical changes occurred in Building 7. That's when they started getting rid of all of the anterooms where people were supposed to change their clothing, and they started cutting those up into little cubicles to make additional offices, because that's also when Huebner started increasing the number of personnel in the Laboratory of Infectious –

BSP: Could you give me number of the viruses that they – roughly? Hundreds?

EB: Oh it was – as a matter of fact, I have the numbers in the article. I'd written them down. It was in the thousands, but I think they isolated I think 20 new individual virologic types of viruses and there were thousands of isolations and there were some viruses that they never even – for instance if they had what looked like a group A coxsackie virus they didn't even bother typing it.

BSP: Then what motivated him to study the cancer virus in '58?

EB: I think this was – I think this was related to the experience he was having with his viruses and the ubiquity of the viruses. He had a very fertile imagination and as I say he had great intuition and he began speculating. He said, "People have these viruses" and he speculated that some of the frequent exposure to viruses could possibly lead to chronic illnesses later on in life, and then he started to say, "Maybe even something like cancer" and then he – you know, back in the early '50s –

BSP: There were some reports.

EB: The animal viruses were becoming established as subjects for study and when polyoma virus was discovered and then Dr. Sarah Stuart and Bernice Eddy started working on them, Huebner also looked at these and said, “These look like good viruses to study in the animal population,” because he felt that in a way the experience in the animal population would mirror what was happening in the human population and that’s when he started his studies on the polyoma viruses and then later on when he became involved in the virus cancer program he was able to recruit Dr. Gardner and Dr. Gardner was able to continue the population study in animals of the various retroviruses of mice.

BSP: I was reading your manuscript; I was quite fascinated to see at NIAID, the Allergy and Infectious Diseases Institute, the cancer research, Huebner’s cancer research, was cut and cut and then generally at NIAID had some difficulties and, at the same time, NCI was getting bigger and bigger and NCI also studied virus cancer problem. And could you say a little bit about the institutional changes? I mean his transfer from NIAID to NCI and the situation of these two institutes.

EB: Well, actually, as I say, Huebner started his cancer work while he was still with LID and ,of course, as I say he was fascinated with polyoma as an animal model that mirrored what he thought – what he though mirrored the human experience. And also he was working with Wally Rowe and Janet Hartley and they also started doing some studies on the animal cancer viruses, but the National Cancer Institute was also becoming interested and they – the embryonic development of the virus cancer program really started toward the end of the 1950s and then, because of the work that Huebner initiated, he – the people at NCI started inviting him to some of the early meetings of the organizations that eventually became the virus cancer program and it was at this point that Bob became more and more interested and, of course, they were very –

[audio break]

EB: – people and he had all kinds of projects that he had thought about ways to perhaps study cancer. He was still involved with the epidemiologic approach, but then early on, I guess it was 1962, when it was discovered that adenovirus type 12 was oncogenic in suckling hamsters that he really became sort of deeply committed to cancer research and of course through subsequent work discovering the tissue antigens and his concept that cancer transforming factor or factors were being transmitted genetically I think stimulated a lot of his work and the interest in the cancer program. He was instrumental in helping set up and organize the virus cancer program and he was the one that was really given responsibility of establishing the widespread network of collaborators all over the country and he was the one really that was one of the prime originators of the virus cancer program and he really worked very efficiently in setting up the whole network and the whole collaborative effort.

Of course the other thing is that with the demonstration that the adenovirus, which was widespread in the human population, was a potential pathogen influenced his attempt to study whether or not the adenoviruses had anything at all to do with human cancer causation. Also, around that time, because of the development of recombinant DNA technology and molecular virology, he became very interested in work that his alma mater was doing in the person on Dr. Morris Green who had the institute of molecular virology, and he collaborated very actively with Morris Green on studying the molecular virology of DNA viruses. Of course nothing much came of that because they never established that adenoviruses were ever a cause of human cancer, but in any event Huebner gradually got into this.

Apparently there were political ramifications of Huebner’s increasing involvement with the cancer activity. There was some turf battles about whether the cancer viruses should stay with cancer or whether they should stay with LID, and apparently there was some marked differences of opinion and some tempers flared at times. As a matter of fact at that time I think Dorland Davis was director of NIAID and he was very uneasy that so much cancer work was being done in NIAID and also the fact that NIAID didn’t have control of all the virus work. So, anyway –

[break in audio]

BSP: So, we talked about Bob Huebner moving to NCI and starting his own program within the larger virus cancer program of the NCI and how did it go? Did it go very well? It started in 1965, right?

EB: Well, actually, I think the program started in 1964.

BSP: ’64, okay.

EB: And he was very actively involved in the program until he finally transferred over, I think it was in 1968. So for three years he was at – it was either ’68 or ’69, he was very actively involved with the program, but he was also still Chief of the Laboratory of Infectious Diseases. He was doing his own laboratory work. He was administering the LID and he was also administering the network of his collaborators who were working on the virus cancer program.

So, actually he would have moved over earlier but there was not enough room to accommodate him and the people that he wanted to bring with him – actually cancer wanted Huebner; they also wanted Drs. Rowe and Hartley, but Rowe and Hartley decided they just wanted to stay in LID and they were still working with cancer viruses, but they were working on them at LID and they were perfectly happy, or at least LID was happy with that arrangement because, you know, after all this was still an infectious agent.

BSP: I see, so at NIAID the cancer virus program, research program, has been going on even after Huebner left?

EB: Yeah they were working on it, but they were also working on other things as well. I think what happened was after Bob Huebner transferred over Robert Channek [spelled phonetically] became chief of the laboratory and I think they set up another laboratory for Wally Rowe, I think it was called the laboratory of viral diseases or something like that. Actually, I think Huebner may have been chief of that for about a year before he moved over and then Wally Rowe took that over and I mean, you know, it's all – I have it all documented in the book.

BSP: Was it an amicable arrangement getting out of NIAID?

EB: Yeah, it was amicable. That is to say that there had been some tension and also some difference of philosophy between Bob and the director of NIAID, but I think they –

BSP: What kind of difference?

EB: Well, it was – I don't think that I –

[break in audio]

BSP: You know the late 1960s after all the genetic codes were deciphered and people got interested in molecular biology, and certainly a virus as a carrier of RNA or DNA became a focus of interest for many people and how Bob Huebner's group started working with the genetic manipulation of the virus in cells and how that led to the hypothesis, oncogenic hypothesis. Could you make any connections with that?

EB: Well, actually, Bob Huebner himself did not do any work with molecular virology or DNA technology. The ones who were doing this actually were Wally Rowe and Janet Hartley. They were interested in this. However, Bob realized that this was going to be the future of virological research and what he did he started recruiting people who had expertise in molecular virology and molecular biology and he started – this was probably in the early '70s. The other thing is that even though he had formulated the oncogene theory from the transmission genetically of the transforming substance which he called – incidentally, he was the one that coined the term "oncogene". The only think is that if this was a substance that was transmitted genetically the model that he was using, namely the RNA viruses and the retroviruses, posed a bit of a problem because, according to the dogma of inheritance, let's see, DNA led to the formation of RNA to message –

BSP: Protein, right.

EB: But RNA did not necessarily lead to DNA so something had to account for the genetic transmission in the chromosome of DNA, and that was the thinking behind the people who discovered reverse transcriptase. So anyway as soon as that came out this opened up a whole new era, a whole new field, of molecular virology, but also this time – this was around 1969 / 1970 Bob realized that the future advances were going to be made in terms of the molecular mechanisms involved in biological research, so he started recruiting people into the virus cancer program who could provide that kind of expertise and he recruited people like Renato Dulbecco, Sam Spiegelman up in New York, and then also he apparently became acquainted with J. Michael Bishop who was at the University California San Francisco, and he recruited him into the virus cancer program.

BSP: To? To NIH NCI?

EB: Well, it was – actually, Bishop stayed out in San Francisco and that's where they did their pioneering work. But Huebner felt that people with the expertise working on the molecular basis of virology were really needed in order to get the program moving, and also he was interested in learning more – finding out about the nature of the oncogene. I mean he was only able to go so far. I mean he could postulate. He could present the hypothesis, but he himself didn't have the capacity to prove it scientifically. He had to get – he needed people who had the expertise so that this might become possible and that's the reason that he enlisted the aid of the people who had this kind of expertise.

As a matter of fact I think it was the 1972 annual report of the National Cancer Institute in the section that Bob was writing as chairman of the virus carcinogenesis branch. The very first sentence he says we have to find more of the basic structure of the oncogene and then he went on to further discussion but also, in the same report, was the first appearance of Bishop as one of the people who had received a contract from the virus cancer program and, as a matter of fact, Bob was the one who really provided major financial support for the project and also he put one of his associates as the project officer to kind of see what was going on. This was Dr. Edward Skolnick who also was on of young people who was becoming very expert in molecular virology. As a matter of fact Skolnick was one of the people who discovered one of the oncogenes – I think the RAS oncogene.

BSP: So, Harold Varmus was one the young –

EB: I beg your pardon?

BSP: Harold Varmus – Varmus the former NIH Director?

EB: Oh, Varmus, yeah. Varmus was an associate of Bishop, but I'll tell you if you want to read something interesting read the interview that Carl Baker conducted with Harold Varmus and Varmus said that when he was in California working with Bishop he absolutely no interest in any of the administrative aspects of the way the lab was run. He had absolutely no idea where the money was coming from. All he knew was that as long as the money was there he was happy, but Bishop was the one that arranged all of the administrative details and I think Bishop was the one who had – he was the one with whom Huebner had most contact and he was the one that the supporters – as a matter of fact, in the paper describing the identification of the oncogene as a normal component of animal cells it lists the contract number that Huebner had arranged for Bishop and his laboratory. One of the things that also was very interesting was the note the Bishop wrote at the time of Huebner's testimonial dinner, retirement dinner, saying that how much he owed Bob for giving him financial support at a very critical time in Bishop's research activity. So, but in any event Huebner recognized that molecular techniques were very important for virological research.

The thing is that in the later part of his career he was working on the immunology of cancer and really I don't think it was very sophisticated research. As a matter of fact the Zenda Committee [spelled phonetically] found that this is the one aspect of the virus cancer program that they thought was not going to be very helpful in trying to elucidate whether or not viruses –

BSP: When does the Zenda Committee setup and give the final report?

EB: I think the final report came out in March of 1974 and basically what they did is they criticized the way the virus cancer program was being administered. I think that probably the best thing is if you look in the chapter where I talked about the Zenda report I outlined all of their objections and then also their recommendation and these were pretty much followed, and it was shortly after that that the virus cancer program began to gradually diminish and, of course, there was the conflict between the way to administer research money – whether to do it through the grant or the contract process and then gradually what happened, they started giving out – contracts were the primary method of financing of the virus cancer program to the various contractors, but then gradually, from about 1975 on, the ratio of the monies between contracts and grants reversed and more grant money was being awarded for cancer research. And then I think the program finally disintegrated back in the early 1980s. I think Vincent DeVita sort of reorganized the NCI and actually a lot of the people who had been involved in the virus cancer program began to feel that viruses, that at least RNA viruses – the retro viruses, were not a very important cause of human virus.

BSP: Human cancer?

EB: Human cancer and so they abandoned the program and they went on to other things, but a lot of the people who had worked on the retro viruses had had sufficient training in that discipline so they went on to become very prominent as AIDS researchers.

BSP: That's right. That's right. Yeah. Yeah. Do you know why the Zenda Committee was setup originally? In other words that people outside, the extramural community, began to fear that the contract system is not really fair.

EB: Well, there was a lot of pressure from the academic community and all of this pressure was communicated to the administrators of the National Cancer Institute. Now this was shortly after the National Cancer Act was formulated and new administrative bodies were set up. I think it was the National Cancer Advisory Board – the NCAB. And it think because of all the criticism and of course a lot of this was directed against Huebner and the way Huebner was administering the program that this new sort of advisory board setup the Zenda Committee to look into the allegations that were coming from the academic community and evaluate them. So, the Zenda Committee worked on – they put out a preliminary report with their recommendations. I think that came out sometime around December of 1973 and then I think they put out a final report in March 1974 which was basically unchanged from their preliminary report and they made the recommendations which they did. Again, I think that I had most of the recommendations in the chapter in the manuscript.

BSP: With the contract system Huebner was in control of giving contracts, was that the –

EB: He was the –

BSP: Or is there a committee –

EB: – chairman of the review committee that was responsible for reviewing and giving out contracts. This was – one of the criticisms was that his review committee consisted mostly of his contractors and they were the ones that were approving the contracts and then the academic community said that most of the contracts were really not very valid scientifically. They were second-rate research, which actually was not true because when you look at the contracts they actually had – they were pretty valid as far as they went.

BSP: Where did the contracts go, I mean, to the industry?

EB: Yeah. I have a partial list of the people in the network, and the other place to find a listing of the contracts was in the annual report of the National Cancer Institute. You have to go down into the belly of NIH – of the NIH library in Building 10 and there's a whole cellar of large shelves devoted to those reports, and usually, as part of the reports of individual branches, they list the contracts that were given out for that particular year. That's where I got the information about the amount of money and number of the contracts and, as I recall, Bishop was the project manager for the report and Edward Scholneck [spelled phonetically] was the project officer. In other words he was back here and keeping it and very frequently Huebner was the project officer for many of the contracts in which he had a personal interest, another one of the objections that the Zenda Committee had the fact that he was project officer for his own contracts.

BSP: I understand that in the '60's – late '60s that cancer virus had a high hope for curing human cancer and you said in the 1980s that hope was almost –

EB: Well, actually the –

BSP: – what – '70s – can you?

EB: I think the main reason is they looked rather extensively for viruses in patients with cancer and never could find any and, as a matter of fact, one of the indirect results of the search for viruses as a cause of human cancer was the discovery of HTLV 1 and 2 that Robert Gallo did. Robert Gallo, apparently, was very much influenced by some of Huebner's ideas about the role of the retroviruses in disease. And what he did – they started looking for evidence of reverse transcriptase in different tissues to see whether or not they could detect any viruses. Finally there was a patient who showed up in the National Cancer Institute in the Clinical Center with the HTLV 1 virus. As a matter of fact Dr. Tom Walden [spelled phonetically] I don't know if you are familiar with –

BSP: Yeah.

EB: He was the one who was taking care of the patient and he brought blood from the patient over to Gallo and I think Gallo had found that he was able to maintain a continuous line of T4 cells and using the cytokine interleukin 2 as a sort of a stimulator he was able to keep the virus line going, and it was from a patient who had the HTLV 1 or T-cell lymphoma that he was able to isolate the virus. And then, later, HTLV 2 which causes hairy cell leukemia with Sezary syndrome, so those were the only ones. Of course, during the 1960s it was recognized there were some viruses that were responsible – some DNA viruses. There was a whole group of herpes viruses that cause cancer. Even though they were probably discovered initially they really have not been a prominent – well I shouldn't say – I'll take that back, because they do cause a certain amount of human cancer like hepatocyte cellular cancer, which is caused by hepatitis B and C, then Burkitt's Lymphoma from the Epstein-Barr virus, the herpes viruses that cause female genital tract disease and then the virus that causes the lymphoma that's in AIDS.

[break in audio]

EB: It's human herpes virus 8. I'm blocking on the name. But so anyway, all of those viruses, but again and in the totality of the incidence of cancer this is just a small proportion. So, I think the theory is that most cancers are probably just due to genetic mutation.

BSP: Could you comment on the development of techniques and evolution of ideas [?] the relationship. You mentioned a little bit in you manuscript – what's leading?

EB: Well the thing is that the research, at least the way microbiological research has been done, well especially with virus research, you have to have a system of living cells to act as a host. So, started of with some of the laboratory animals. You had mice, rabbits, guinea pigs and then, of course, monkeys and these were the major laboratory animals with which the early investigators worked back in 1933. I think they found that ferrets could be used to grow influenza virus, but then the next major development as far as viruses were concerned – of course, I'm not mentioning any of the usual bacteriological techniques, just for viruses. The next major technique or laboratory host was the embryonated egg because with the egg you could grow influenza, you could grow the Rickettsias you could make vaccines using this.

BSP: Outside the living organism?

EB: Well, no – well, the embryonated egg is living so it's – so when I arrived at NIH, as I say, we had the usual laboratory animals, the rabbits, the guinea pigs, the mice and then the embryonated eggs and then the next major laboratory host that appeared was the suckling mouse – yeah the suckling mice.

BSP: Suckling mice?

EB: Yeah.

BSP: What is it?

EB: For growing coxsackie viruses and then also, of course, they were used to grow the virus leukemias.

BSP: Is tissue culture techniques is the –

EB: Tissue culture technique was the latest development and, as a matter of fact, tissue culture techniques started in the National Cancer Institute going back many years around the – I think Alexis Carrell was with the Rockefeller Institute, but Wilfred Earl [spelled phonetically], in the Cancer Institute, had been experimenting with tissue culture techniques for many years, but he was using these large containers and using large pieces of tissue and it was only during the late 1930s / early 1940s that they developed the other techniques where they would use specific cell lines growing in nutrient fluids, and using monolayers of cells, to grow viruses that most of the modern virology was able to develop.

As a matter of fact, as I say, up until Dr. Shelokov [spelled phonetically] – Dr. Shelokov [spelled phonetically] I mentioned before. He and Dr. Coal [spelled phonetically], my medical classmate, and I were interns together at Boston, but when Dr. Shelokov [spelled phonetically] came down with Dr. Habel, Dr. Habel sent him off to Dr. Enders's laboratory at Harvard – well actually it was Children's Hospital – to learn the technique of tissue culture in roller tubes, where having a monolayer of living cells, and then what happened is that various cell lines were developed – self sustaining cell lines that could reproduce themselves.

One of the other major developments was the development of the nutrient solutions that were used to bathe these cells to keep them healthy for a week or ten days until the fluids had to be changed and Dr. Harry Eagle – I don't know if you know that name; I think I mentioned that – devised was known as the Eagle Special Medium, the combination of amino acids and minerals and also I think he threw in a little protein, calf serum of various types. So, these were the techniques that were used to propagate viruses and, of course, this is the development that led to the successful development of the polio vaccines.

BSP: I see. In one other chapter you mentioned that it is the public health concerns, not the techniques, that are leading the research direction.

EB: Well –

BSP: Is it?

EB: Well, at least in virology it was the techniques that determined what you could do. As a matter of fact, with the availability of tissue culture brand new viruses were discovered. In the manuscript I mentioned the so-called ECHO viruses. These were viruses that were just isolated in tissue culture through no other animals. For that matter – well, the coxsackie A viruses, for instance, grow almost exclusively in suckling mice and most of them don't grow in tissue culture. Some of the group B viruses will grow in tissue culture, but the ECHO viruses and things like the real viruses, the cytomegaloviruses, these were all discovered in tissue culture.

As a matter of fact, this was one of the things that happened during the Junior Village study because they used tissue culture almost exclusively and they discovered all of these other viruses. A lot of ECHO viruses they discovered a new – what was then described as a relatively new group of viruses actually discovered by Sabin called the real viruses. Also, Wally Rowe was one of the three people that almost simultaneously discovered the cytomegaloviruses. These were isolated in tissue culture.

BSP: Going to the '70s recombinant DNA technology was developed and then molecular biology was well-used [?] by that time and I met many biochemists, molecular biologist, just technique, what is the view from the biologist? Is it a molecular biologist's technique or very integrated –

EB: Well, it's integrated. I mean you use whatever technique is appropriate for what you're trying to do. If you're going to grow a virus you have to use tissue culture. If you want to study it genetically use DNA technology. It all depends what you're trying to do. If you don't have the technology I mean you're limited in what you can do. So, part of the research is finding new tools depending on what you're looking for and what you're trying to accomplish.

BSP: Right. Could you finally comment on the general NIH context for Bob Huebner's work and Charles Armstrong's work and your work and generally it's growing importance, or are there other things that you'd like to say?

EB: I think Bob – well, Dr. Armstrong,I think is a – they were pioneers. They provided shoulders for other people to stand upon. They made very important fundamental observations. I think Dr. Armstrong remained sort of – had a biological approach throughout most of his career because of the limitation of the tools he was working with. I mean he really never – I mean his active research career stopped probably in the mid-'40s, and this is before some of these techniques became available.

Bob Huebner started off the same way, but I think that Bob Huebner grew in terms of the sophistication of his techniques and what he was able to do in the field of virology beside isolating many viruses I think he also made some very important contributions to the nature of virology and also I think the – his contribution to the pathogenesis of cancer was very important because despite the fact that he didn't discover the fundamental nature of the oncogene, he was the one that conceived of a group of genes that directed the development of cancer and he stimulated enough interest in this so that other people did go on and develop some of the other very fundamental knowledge in understanding carcinogenesis. But he was the one that I think really initiated the concept – as a number of authors have said that they spend many years just chasing Huebner's ideas.

BSP: That's great. Thank you very much.

EB: Okay. Is –

End of transcript